Psychological Bulletin

ALCOHOL: A CRITICAL REVIEW OF THE LITERATURE, 1929–1940

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It is somewhat more than 10 years since the *Bulletin* last published a review of the literature on alcohol. In that time an increased interest in the subject can be noted. Some of this interest is directly referable to the effect of repeal or modification of prohibition, both in the United States and abroad; but much of the recent work has been the result of continued interest in more strictly experimental problems. The three abstract services which were consulted in the preparation of this study (*Biol. Abstr.*, 1930–1939; *Psychol. Abstr.*, 1929–July, 1940; *Soc. Sci. Abstr.*, 1929–1932) listed a total of some 300 titles on the subject. Those articles which were totally unavailable were necessarily dropped from the list, together with those which were entirely propaganda; of the remaining articles, 135 have a direct bearing on the question of alcohol in man.[1]

Factors to be considered in setting up an experiment on the effects of alcohol include the method of administration, dosage, concentration, and tests for alcohol in the system.

METHODS OF ADMINISTRATION

A very brief acquaintance with the literature in the field of methods of administration serves to bring out the fact that much experimentation, both on animals and on humans, is of very limited value because of lack of uniformity in procedure. The psychologist can learn much about technique from the work of experimenters in the animal field. In work with the smaller ani-

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[1] While the review covers the period 1929–1940, certain articles published in 1927 and 1928 are included. In the main, these are foreign articles which were not covered by the 1929 review in the *Bulletin*; a very small number have been repeated from the previous publication because they seemed necessary to the development of some topic.

mals the tendency has been to expose the animals to alcoholic fumes, using a fume tank. This was done by Varner (119) and Hanson and Cooper (54) with rats, and by Durham and Woods (37) with guinea pigs. MacDowell and Lord (71) exposed mice to fumes by putting them into milk bottles. It is impossible to find any very definite standard of alcoholization with this method. The usual procedure seems to have been to expose the animals to alcohol "until they lose consciousness"; Danforth (33) kept his mice in the fumes for one hour, twice a day, in order to establish standard conditions. This is probably longer than the time necessary for loss of consciousness as measured by failure to react to stimuli.

Chaudhuri (29) followed the example of Bluhm (14) in injecting his mice, commenting, as does Danforth, that the fume method does not result in deep enough alcoholization unless the experimenter keeps the animals exposed to the fumes for a considerable period. Branch (21) gave alcohol orally to rats by dropper. Miller and Miles (88) injected rats intraperitoneally. Speidel (107) immersed tadpoles in a dilute solution of alcohol. Hausmann (56) offered rats simultaneously water, customary diet, and a solution of sugar with ethyl alcohol, and found that they would take limited amounts of the alcohol solution. Robertson and Stewart (98) administered alcohol to rabbits per rectum, stating that the presence of food in the stomach, even 24 hours after eating, hinders absorption. Harger and Hulpien (55) gave alcohol to some of their dogs by stomach tube and to others intravenously.

Most human experimentation has followed the practice of Widmark (127) of giving alcohol by mouth. Alcohol for oral administration is usually flavored, in spite of the complaint of Echalez (38) that experimenters rely upon a solution of alcohol and distilled water, which he condemns as a "horrible beverage." When the subject is informed as to the nature of the experiment, he is given whiskey, cognac, whiskey and soda, or other standard drinks. When the experimenter wishes to conceal the nature or concentration of his dose, he has recourse to various more or less successful experiments. Erlacher (40) gave to school children a solution flavored with raspberry syrup. Fleming and Goldman (45) mixed the dose with grapefruit juice. Cattell (26) sought to "veil" the presence of alcohol with a mixture of peppermint, lemonade, and ginger. Scofield (104) produced a mixture of alcohol, lemon juice, orange juice, carbonated water, bitters, tabasco sauce, and pow-

dered sugar, in which his subjects still detected the presence of the alcohol.

In order to avoid all chance of recognition, Newman (91) gave his patients intravenous injections of alcohol in physiological salt solution; Miles, in his original study, administered alcohol per rectum.

DOSAGE

There has been equal variation in the amounts of alcohol used for experimentation. Branch (21) gave her rats 1 cc. a day; single doses for rats may vary from Miller and Miles's (88) 1.0 g./kg. to Lévy's (70) 2.0 to 4.0 g./kg. Mice were given 0.2 cc. of 15% alcohol by Bluhm (16). Newman and Card (92) gave dogs 4.0 cc./kg.; and Robertson and Stewart (98) gave 5.0 cc./kg. to rabbits. Koögerdal (66) killed cats wtih a lethal dose of 15 cc./kg. Mohr (89) points out that the degree of alcoholization in animal experimentation far exceeds that known in man, and he feels that conclusions based on such experiments are therefore not to be applied to humans without verification.

In work with humans, some experimenters have left the dose to the option of the subject. Erlacher (40) gave school children 10 cc. every other day; Cattell (26) gave 10 to 20 g.; and Scofield (104), Vernon (121), and Mayerhofer (78) used amounts up to 40 cc. Strongin and Winsor (111) gave 40 to 100 cc., and Graf (52) 40 to 170 g., of alcohol; Bahnsen and Vedel-Peterson (8) gave 100 cc., and Heise (57) 30 to 150 cc., of whiskey.

As in animal experimentation, the dose for humans is often determined in terms of body weight; it may vary from Wierenga's (128) 0.25 to 0.6 cc./kg. to Newman and Cutting's (93) 0.5 to 1.5 cc./kg. This last dose was injected gradually over a one-hour period. In general, single doses are a little larger than those repeated over several days. Newman (91) recommends basing the dose on surface area rather than on weight.

CONCENTRATION

The composition of the solution used is determined by the mode of administration. Animal experimentation uses alcohol in a wide range of dilutions. Newman and Card (92) state that men, dogs, and rats have a constant rate (β) for absorption of alcohol regardless of the concentration, while rabbits and fowls absorb alcohol at a rate proportional to the concentration.

Physiological Tests for the Presence of Alcohol in the System

Stimulated in great part by the problem of alcohol as a cause of accidents, there has been continued interest in the questions: (1) How is the amount of alcohol present in the system of a given individual determined? (2) What is the significance of such determinations in terms of intoxication? (Gerendasy's (51) summary of the tests in use in Great Britain, Denmark, France, and the United States has not been available for this report.) Using the blood concentration as a measure, Johnson (62) established that at 4 mg./cc. all his subjects were "dead drunk," or in a stupefied mental condition. Three mg./cc. was associated with mental confusion; less than 3 mg./cc. was associated with exaggeration of mood. Johnson examined 200 patients, using Bogen's technique of a color standard. Heise (57) also tried to define "intoxicated" in terms of an objective measure. He found alcohol in the urine up to 0.02% after 30 cc. of whiskey, and alcohol up to 0.10% after 150 cc.

The period during which the effects of acute alcoholization can be studied is still a matter of some discussion. The questions to be answered are: (1) What length of time is necessary to get the maximum effect? (2) What is the duration of that effect? Widmark (127), using 23 men and 10 women as subjects, plotted the curve of alcohol in the blood after the ingestion of known doses. He varied the conditions (rest, exercise, fever) and the method of administration (alcohol alone, with a large amount of water, and with a standard meal), using only two or three trials for each special condition. Widmark confirmed Mellanby's earlier conclusion (82) that the rate of absorption of ingested alcohol into the blood stream is constant for the individual and that it takes place within about two hours. Haggard and Greenberg (53), on the basis of experimental work with four dogs, felt that they had disproved these results. They declared that ingested alcohol is not completely absorbed for more than six hours. Harger and Hulpien (55) gave fasting dogs 3 g. of alcohol by stomach tube. Analysis of the entire gastrointestinal tract at various intervals thereafter showed virtually complete absorption of the alcohol in two hours.

Widmark (127) also established a constant (β) for the disappearance of the alcohol from the blood stream; Haggard and Greenberg (53) assert that the rate of oxidation is proportional to the amount of alcohol present in the body. Newman and Cutting

(93), with 20 human subjects, after a single intravenous dose injected so that the administration required one hour, present a curve of blood alcohol plotted against time which is a straight line, but which varies considerably from individual to individual.

There would seem to be no fixed number of hours during which the effect of alcohol can be said to persist. Winsor and Strongin (130), in a preliminary study, found increased parotid secretion in humans only during the time the alcohol was in actual contact with end-organs located in the walls of the digestive tract. During the period when the subject reported feelings of intoxication there was marked inhibition of secretion. In a later paper (111) the same authors stated that this inhibitory effect comes in waves, beginning about 45 minutes after ingestion of alcohol and still evident after a five-hour period.

A considerable amount of work has been done on the question of *individual differences in response to alcohol*, together with factors, other than inherent ones, which might serve to affect the response. Strongin and Winsor (111) found great difference in tolerance in their subjects. In an attempt to secure "a desired effect" they used doses of ethyl alcohol varying from 45 to 75 cc. for different individuals; one man showed no reaction on 100 cc. They felt that these differences were due not only to weight, age, and previous drinking habits, but also to what they called "general competence." Y. Y. (Robert Lynd) (135) attacks this assumption that a "good head" for liquor is associated with heroic traits; he cites examples of leaders who have been poor drinkers and concludes that the older idea is dying out, even in the Nordic countries where it had found most acceptance.

The nature of tolerance and habituation is still not clearly understood. Adams (3) lists nine different theories of tolerance for drugs, of which seven assume biological changes in the organism, and two assume physiological changes. Binswanger (12) reported 174 cases of psychopaths who were inordinately affected by small amounts of alcohol. Atzler and Meyer (7) seem to feel that some of the differences in tolerance ascribed to habituation may be subjective in origin. Hausmann (56) found that the spontaneous activity in rats was not affected by the consumption of alcohol solution so long as this was optional, but as soon as they were forced to drink it their activity decreased.

Lévy (70) injected groups of rats with small doses of alcohol. Her normal rats were anaesthetized with 2.16 g./kg.; they died

after a 4.0 g./kg. dose. On the other hand, her habituated rats showed no anaesthesia on doses up to 2.56 g./kg.; they were anaesthetized by doses ranging from 2.72 to 4.0 g./kg.; none of them died.

Newman and Card (92) experimented with three habituated dogs and three controls. The dogs were given 4 cc. of alcohol per kg. body weight by stomach tube. The habituated animals showed blood alcohol curves which rose higher and dropped more rapidly than those for the controls; the habituated dogs "became much more intoxicated in a much shorter time" and recovered more rapidly. The authors noted that the mechanism for acquiring tolerance for moderate concentrations of alcohol does not provide tolerance for anaesthetic concentrations; this is in disagreement with Lévy. They are certain that tolerance is not a matter of delayed absorption or increased oxidation; "it will be necessary to investigate further the possibilities that it may be due to changed nervous tissue permeability, increased cellular tolerance, or psychomotor compensatory mechanism" (92, p. 440).

Miles (87) has pointed out that for weaker concentrations of alcohol (up to 4% by weight or 5% by volume) the blood concentration induced reaches only to 60 or 70% of that which results from an equal amount of alcohol in a beverage of 20% concentration or greater. To this extent the concentration of the beverage must affect tolerance.

Sex, oxygen, work, and food have all been studied with respect to their effect on tolerance. Widmark's (127) work cited above showed no changes in the individual constant for disappearance of alcohol from the blood under varying conditions of food or work, although there was a sex difference in the range of these constants. Using seven subjects for 10 trials each, Nyman and Palmlöv (94) concluded that alcohol taken by mouth on an empty stomach disappears at the same rate during exercise and at rest. Meyer (84) is reported as finding no difference in the rate of oxidation of alcohol between the habitual drinker and the control, either under basal conditions or when doing measured work.

Abderhalden and Wertheimer (1) reported that rats fed a rich protein diet are more capable of resisting the effects of alcohol than are the carbohydrate-fed controls. This difference was demonstrable both in psychomotor function and in physiological resistance. The same authors (2) injected groups of mice with 0.2 cc. of 30% alcohol daily until the animals died. Each group con-

tained three or four males and three or four females, paired as nearly as possible for weight; in each group the males had all died before the last of the females.

McFarland and Barach (80) reported a relationship between oxygen want and alcoholic intoxication, and later Barach (9) found that the inhalation of oxygen produced temporary improvement in the symptoms of alcoholization, while the presence of 7 to 10% of carbon dioxide in the oxygen resulted in a permanent improvement.

PHYSIOLOGICAL EFFECTS OF ALCOHOL[2]

Lévy (70) killed groups of four to six rats at various intervals from one minute to four hours after administration of 2.16 g./kg. of alcohol. Analysis of the blood, brain, kidneys, and total body showed no real differences in alcoholic concentration in any organ compared with the animal as a whole.

Robertson and Stewart (98) sectioned the brains of rabbits killed 15 minutes to 4 hours after the administration of alcohol per rectum. They found a gradual increase in the oxygen uptake in the brain tissues of those animals which had been killed in the first 30 minutes, continuing for about an hour and followed by a gradual fall. The increase was largest in the tissues composed largely of grey matter.

Two good summaries on the *physiological effects of alcohol on man* have been published by Himwich (59) and Bogen (19). Although intended for the educated layman, these serve to bring together much experimental work on the absorption, distribution, oxidation, and excretion of alcohol, and on the physiological effects of intoxication and chronic alcoholism. Bogen and Hisey (20) have also published a book for use in high schools, which carries a good bibliography.

In an article for laymen, Martland (77) summarized the literature on the pathology of alcoholism. He stated that the main damage is done to the central nervous system directly; the effect is mainly functional, and structural changes are not demonstrable. Varner (119) felt that his own results were in accord with this statement. His rats showed greater loss in a complex maze habit than in a simple one; the rats seemed to acquire a resistance to the effect of alcohol.

[2] Not all of the work on the physiological effects of alcohol can be treated in this review. It has been necessary to limit the attention to general summaries and to articles on differential distribution and the effect of alcohol on brain tissue.

Warner (126) disagreed with Martland's conclusions. From a study of seven cases, together with the literature, he pointed out that histopathological changes do occur in chronic alcoholism, but that they show great variation both in kind and in distribution, and that there is no correspondence between severity of clinical symptoms and degree of brain change. Toulouse (114) described encephalitic lesions in acute alcoholic psychosis. Cowles (31) has found some cases in which alcohol sets up a meningeal edema, which he says leads to a greater susceptibility to alcohol and a disintegration of the personality.

Malapuu (74), according to an abstract, concluded that alcoholism, acute or chronic, increases the permeability of the meninges, and that the pia mater is most affected. Thomas (112) demonstrated that alcohol causes dilatation of the pial arteries, increasing the flow of blood through the brain; the effect is independent of changes in systemic arterial pressure. Koögerdal (66) produced acute alcoholic poisoning in cats with a dose of 15 cc./kg., resulting in death within half an hour or an hour. Likewise he gave rabbits 5 cc./kg. every other day for from 152 to 280 days. Both the acute and the chronic cases showed edema of the brain and macroscopic, as well as microscopic, hemorrhages in the capsule and brain substance.

PSYCHOLOGICAL EFFECTS OF ALCOHOL

Miles's summary of the psychological effects of alcohol in man (87) includes a bibliography which lists a few titles later than those cited in his previous book, *Alcohol and human efficiency.*

Chauchard, Chauchard, and Kajawara (28), Courtois and Néoussikine (30), and Varé, *et al.* (117) have all done work on the effect of alcohol on chronaxy. None of these articles has been available for examination except in abstract. Beringer and Ruffin (11) determined that the pressure limen on 20 chronic alcoholics lay toward the upper normal limit; in most cases there was lability of the limen. Ruffin (100) also found that, particularly shortly after deprivation of alcohol, chronic alcoholics evidence rather marked instability of chronaximetric cutaneous and visual limens, with a tendency to increase with successive stimulations. Malamud, Lindemann, and Jasper (73) gave 200 cc. of 25% alcohol to each of nine male subjects; all of them showed changes in the normal flexor-extensor chronaxy relationship: equalization or reversal seemed to be associated with pronounced coördination dis-

turbances; exaggeration of the normal differences was linked with acute mental changes (talkativeness, excitement, confusion, and combativeness).

Among specific studies in the sensory field, Sterzinger (**108**) found that alcohol made time-intervals seem longer than they really were; Mullin and Luckhardt (**90**) reported that alcohol caused a distinct decrease in pain sensitivity, but had little effect on sensitivity to touch. Bromberg (**22**) found tactual aftereffects prolonged in alcoholic psychosis in such a manner as to suggest that they may serve as bases for hallucinations. Leroux and Caussé (**68**) and Epstein (**39**) noted vestibular disturbances at all stages of chronic alcoholism.

Scott, Scott, and Luckhardt (**105**) used the balloon-in-stomach method of recording hunger contraction; their six male subjects also recorded subjective hunger sensations. Alcohol did not affect the hunger period except for some delay in onset of contractions; the contractions were similar in the alcohol and in the control tests, but there was marked increase in hunger sensations reported. The authors suggest that alcohol depressed the higher mental functions, increasing the subjects' ability to sense hunger contractions.

Scofield (**104**) used the Dodge mirror recorder to register photographically the increasing inadequacy of the pursuit phase of ocular nystagmus as the amount of ingested alcohol increased. This was a preliminary study with five subjects, who were given concealed doses of 15 to 45 cc. of alcohol. The author considered the conditions of his experiment unsatisfactory.

In a study by Gantt (**50**) on five dogs with well-established conditioned salivary reflexes, graduated doses of alcohol (0.5 cc. to 4.0 cc./kg.) increased the latent period of the response in a degree proportional to the dose; and the intensity of the conditioned reflex was in inverse ratio to the amount of alcohol. The authors point out that this confirms Dodge and Benedict's conclusion that alcohol never shows any stimulating effect and that the depressant action is proportional to the dose. However, Santesson (**101**) has reported the failure of alcohol to depress an inhibition in the case of frog's heart muscle inhibited with muscarine, and therefore suggests that it is not correct to dogmatize that alcohol is always a cerebral depressant.

Herren (**58**) found changes in tremor rate "in some instances" after ingestion of alcohol in amounts determined by the inclination of the subject. He reports only five cases, all male.

Alcohol caused a disturbance in steadiness and motor coördination, measured in terms of ability to keep a beam of light shining continuously on a photoelectric cell. In this experiment, Strongin and Winsor (111) varied the dose of alcohol in an attempt to approximate an equal effect on the subject. They made no statement regarding number of cases or exact procedure. Graf (52), using two subjects, had them lift a weight of 15 kg. one meter during a 10-minute period, followed by five minutes of rest. The subjects were given alcohol, from 40 g. to 170 g., and the alcoholic concentration of the blood was determined by Widmark's method. Decrease in accomplishment was not directly proportional to blood alcohol, but approached more nearly a quadratic function. Efficiency was slower in returning to normal than was the blood content.

Markiewicz (76) ran a large number of white mice on a three-story maze; his alcoholized mice "showed a narrowing of scope of behavior." Miller and Miles (88) injected alcohol into rats which had overlearned a maze. Five minutes after injection there was marked decrement in speed of running and considerable increment in the coefficient of variability of the time scores; the rats showed an increased tendency to make errors in goal-pointing blinds; there was regression to an earlier distribution of errors.

In connection with research on the effect of alcohol on driving skill, Bahnsen and Vedel-Peterson (8) and Schmidt (102) undertook tests of some psychomotor functions. Using some 50 subjects, they tested reaction speed, coördination, and ability to concentrate after ingestion of a fairly large drink of whiskey and soda. They found a correlation between the concentration of alcohol in the blood and psychological effects; the psychological effects fell off more rapidly.

Tuovinen (115) served as his own subject in an attempt to determine the relative effect of varying concentrations of alcohol. He took doses of 40 cc. of alcohol in concentrations from 4% to 60% and then tested his ability to thread needles. He ran a formal series for 54 days during a period of 10 weeks, alternating 4 days without alcohol with 5 days with alcohol of a given concentration. He concluded that "a strong concentration of alcohol has more effect on a system unused to alcohol than does a weak one."

Erlacher (40), continuing a previous study on boys (41), gave 10 cc. of alcohol to girls every other day for 10 days. Both 10- and 14-year-old girls lost efficiency in reading, calculation, and bead-stringing. The effect lasted an hour and a half and was more

noticeable in these girls than in his previous group of boys. Using three men and three women, Kurka (67) studied the effect of "moderate amounts" (4.43% to 39.97% by volume) of alcohol on a variety of psychomotor functions. He reported that ability to form concrete concepts was improved; all other functions were affected unfavorably, including ability to form abstract concepts. The abstract gives no statement of the statistical significance of his results.

Mead (81) used six subjects in a study in which he compared the ability of occasional drinkers to learn an artificial language after concealed doses of alcohol with their performance under control condition; at the same time he conditioned the same subjects to retract the finger to a light by means of an electric shock. He concluded that "30 cc. of ethyl alcohol exhibited a more deleterious effect on higher mental performance than on a simple reflex learning" (p. 20).

The only animal experiment designed to investigate the effect of alcohol on the higher mental functions seems to be that of Fromherz (48). He used three white mice, making his criterion the ability to orient themselves toward a definite color. There were no changes in behavior on 0.2 cc. of 25% alcohol, but increased incidence of failure followed the administration of 0.4 cc. He concludes that this behavior is much less susceptible to the effect of alcohol than are the lower motor functions.

On the basis of three human subjects, Varé (118) states that alcohol has an injurious effect on psychomotor functions and particularly on the choice reactions, lengthening time and increasing errors; it also diminishes voluntary attention.

Fleming and Goldman (45) set up an elaborate experiment in which 17 subjects and 4 controls added 12 sets of digits, each of 50 pairs, presented by a tachistoscope at intervals over a three-hour period. The experimental cases were given 0.6 to 1.0 cc. abs. alcohol/kg.; blood samples were collected throughout the series. The results are unsatisfactory because of wide and unpredictable individual variation in speed of addition, as well as variation in the same individual at different times. The authors concluded that the effect of their small dose of alcohol is slight, if present at all; they recommend further research with better controlled conditions.

A group of 25 men and 25 women were given parallel forms of memory and intelligence tests on five consecutive days by Cattell (26). The series were preceded by concealed doses of 10 g. of al-

cohol, 20 g. of alcohol, plain water, or caffeine. Individual differ-
ences overshadowed all other results. Cattell felt that the experi-
ment would be worth repeating, if one could control age, constitu-
tion, sex, and daily regimen. Seward and Seward (106) believed
that Cattell's dose of alcohol was too small to give positive results.
The Sewards did control sex, education, and drinking habits in
12 men whom they tested for judgment, immediate recall, and
reading time in relation to alcohol in the urine after ingestion of a
single dose, 0.75 g./kg. No general effect on judgment could be
established; recall and reading rate were somewhat impaired.

Using standard intelligence tests, Mahan (72) has measured
loss of mental ability in 50 chronic alcoholics and concluded that
quantitative measurement is possible. Vogel (122) found that his
46 adult white male chronic alcoholics were not significantly more
suggestible than 100 controls on the Hull postural sway test.

Newman (91) describes changes in overt behavior, mood, and
certain other personality factors after a concealed dose of alcohol
given intravenously.

Fleming (44) points out that while the concentration of alcohol
in the tissues is the ultimate measure of its pharmacological effect,
certain elements in the clinical picture of drunkenness contributed
by the personality have been overlooked. Wierenga (128) has
made a beginning on this problem. He tested two subjects of
widely differing temperament and reported significant differences
in susceptibility to the same dose of alcohol in terms of body
weight. He also recommends the use of psychotechnical criteria
in determining degrees of intoxication.

The past 10 years have seen great effort expended in attempts
to find an explanation of the *nature of predisposition to alcoholism*.
Because of our limited progress in objective measurement of per-
sonality, much of the literature in this field is nonexperimental and
nonstatistical. In fact, from some 40-odd titles read under this
topic, only eight are in any sense based upon an analysis of data
on groups of cases, and the majority of these depend upon anamne-
sis for their material.

Wall has reported analyses of the psychiatric histories of 100
men (123) and 50 women (124), of whom the majority had psy-
chopathic personality or "alcoholism with psychosis." He con-
cludes that for both sexes "alcohol offers an escape to the blissful
state of infantile omnipotence." Curran (32) studied 50 women at
Bellevue, of whom 16 were nonpsychotic. He found marked evi-

dence of maladjustment. Tillotson and Fleming (113) have recently announced an extensive research into the possibility of predicting the success of treatment on the basis of a study of the personality and history of the patient. They have reported a preliminary survey of 100 men and 20 women and have drawn some tentative conclusions.

Binswanger (12) reviewed the records of patients in his own private institution and concluded that 77% showed psychopathic inheritance. Carver (25) did not find alcoholic inheritance in even one-third of his cases, including remote relatives. It appeared from a study by Bühler (24) that no one of the Kretschmer body types is characteristic for alcoholics. A statistical study by Wiersma (129) indicated that alcoholism was associated with "constitutional inferiority, low intelligence, and tendencies toward nervous and amorphic temperament and toward criminal behavior."

The first statistical approach to the study of the personality of alcoholics may be credited to Wittman (131, 132, 133, 134). On their responses to certain current scales of personality and environment, she sought to compare 100 alcoholics "without psychosis" with 100 volunteers equated as to age, education, and nationality. Her conclusions support to some extent the armchair assumptions of the medical writers; perhaps her most important conclusion is that the group is not homogeneous, either in developmental background or in personality.

By far the greater part of the literature on etiology in alcoholism is characterized by definite statement of theory, usually on the basis of a more or less extensive experience in the treatment of alcoholics either medically or by the use of psychotherapy or psychoanalysis. Thirty-five articles of this nature, with no statistical treatment even of historical data, advance about as many different theories. At the risk of the same danger of oversimplification to which these articles often fall victim, it is probably enough to say that the authors group their suggestions about half a dozen causes: inability to meet the demands of adult life; feelings of inferiority on the basis of educational, social, or marital inadequacy; parental dominance, or overindulgence; oral erotism; homosexuality; and allergy to alcohol.

There have been several summaries of our present knowledge of *hereditary damage due to alcohol*. Frets (46, 47), Mohr (89), Stockard (110), and Davenport (34) have considered the question of possible germ-cell damage. All concluded that no definite evi-

dence has been advanced, but that, unfortunately, this is not conclusive because investigations have been inadequately controlled. Bluhm (17) feels that there is danger of exceeding our facts in interpreting our present knowledge of this question.

Bluhm's experiments with mice (14, 15, 16) covered eight generations and over 32,000 offspring. Starting with a closely inbred series, the males of 114 pairs were treated with injections of 0.2 cc. of 15% ethyl alcohol solution six days a week; 114 similar pairs were carried as controls. Chronic alcoholization of the males resulted in what she concluded were definite mutations (sublethal genes) induced by the alcohol. In the earlier generations the alcoholized strain showed increased infant mortality. In subsequent generations the mutation produced a definite decrease in this mortality. Branch (21) reported a brief series which indicates that alcohol lessens the reproductive power of rats. In her cases the infant mortality seemed to be associated with feeding problems which were not corrected. Durham and Woods (37) carried guinea pigs through four generations, resulting in 6309 descendants of their alcoholized series and 674 controls. Their results in general showed no defects which could not be explained by factors other than the alcoholization (*i.e.* germinal defects in the original stock and inbreeding). The sex ratio was not altered.

From some experimental work with geese, mice, and chickens, Stieve (109) made elaborate deductions as to the effect of alcohol on the gonads. Accepting a now discredited theory of oögenesis, he suggested that ova are more susceptible to lasting damage from alcohol than are spermatozoa. Waller (125) reported histopathological changes found in nine selected autopsies of men dying during, or immediately after, a period of severe alcoholic intoxication. Parenchymatous degeneration in the testes leading, when sufficiently marked, to practically complete aspermatogenesis was found in each of the nine men. The cell damage was such as to support the placing of clinically recognized, and experimentally induced, alcoholic blastophthoria upon a morphological basis.

Several studies of the offspring of groups of known alcoholics have been reported. Malzberg (75) has pointed out that, during a three-year period in New York, male first admissions with alcoholic psychosis were associated with a lower marriage rate than that of the general male population. Brugger (23) found that in his cases more of the wives of institutionalized alcoholics were feebleminded, schizophrenic, and psychopathic than chance would

lead one to expect. Both of these factors would, of course, complicate the hereditary picture in an unknown degree.

Panse (**95**), in Berlin, investigated the offspring of about 100 men who had fathered children during a prolonged period of heavy drinking, and also in the prealcoholic and postalcoholic periods. He found little direct evidence of alcoholic injury to the germ plasm, but felt that the possibility could not be entirely ignored. The same indefinite results were the outcome of Brugger's (**23**) analysis of the children, nephews, nieces, and grandchildren of 225 institutionalized chronic drinkers. In a rather more extensive study of 728 institutionalized alcoholics, Gabriel (**49**) found no definite evidence that alcohol caused germ-cell damage, although he felt that it might be a factor in bringing out defects already inherent in the germ plasm.

Continuing the work on alcohol and sex ratio, Chaudhuri (**29**) repeated Bluhm's 1921 experiment with mice and found a significant difference in sex ratio following subcutaneous injections of alcohol in the male parent. He felt that MacDowell and Lord's (**71**) failure to get the same results may have been the result of insufficient alcoholization, due to differences in technique. Fetscher (**42**) reported a study of 537 children of drinking parents among whom the sex ratio was 160:100, as compared with a normal of about 107:100. He considers this fairly good evidence of modification of the germ plasm by alcohol; he does not feel that it indicates that alcohol can produce mutations, although it seems likely to him that it may. Gabriel (**49**) studied a smaller group from two separate institutions. The group in an alcoholic institution (presumably without psychosis) had a total of 680 children, with a sex ratio of 126:100. Another group of alcoholics in a psychiatric institution had a total of 414 children, with a sex ratio of 107:100. Approximately 23% of the pregnancies in the first group, and 14.6% in the second group, did not come to term. This fact is associated with families somewhat smaller than the average in Germany for the same social group.

Little of statistical worth has been published on the effect of alcohol on the intelligence of the offspring. Pohlisch (**97**) studied all the offspring (146 in all) of 58 men who had been committed for delirium tremens after prolonged heavy drinking. He examined data from other studies as well and arrived at the conclusion that there was no positive evidence of damage to intelligence as an effect of parental alcoholization available at the time of his study.

Hanson and Cooper (54) used as test material the third non-alcoholized generation of rats in a series whose ancestors for 10 successive generations had been subjected daily to heavy doses of alcohol fumes. The differences in learning ability between 104 test rats and a control group were statistically insignificant.

Tuppa (116) tested the intelligence of 583 Viennese pupils from the third grade up. Part of them came from a drinking quarter of the city, the others from a neighborhood relatively free from alcohol. He found the latter group decidedly superior in intelligence. No reference is made to other socioeconomic factors which might have been operative.

THE RELATION OF ALCOHOL TO ACCIDENTS AND CRIME

Even before the repeal of prohibition the United States became interested in the relation between the use of alcohol and the incidence of traffic accidents; laws were passed making it a very serious crime to operate a motor vehicle while under the influence of liquor. It is interesting to note that a recent summary in the *U. S. Law Review* (6) quotes court decisions extending "operating" to include not only steering an automobile while being towed or pushed, but also starting, or attempting to start, the motor, or even manipulating the gears.

Mayerhofer (79) published a summary of various experiments of his own on the effect of alcohol on the behavior of automobile drivers, from which he concluded that much slower and increasingly variable reactions are consistently shown after the use of alcohol. Miles (87) described the effect of alcohol as it interferes with driving in terms of poorer attention, slower and more variable responses, and increased self-assurance. DeSilva's (35) bibliography lists approximately 325 titles concerning the causes of driving accidents; in 31 of these titles the implication of alcohol is evident.

An anonymous writer in the *Literary Digest* (5) quoted the National Safety Council as saying that the use of intoxicating liquor is not a major cause of traffic accidents; two years later Durfee (36) quoted the same authority to the effect that, in 26 states in which the report of an accident bears the notation "had been drinking" or "intoxicated," 7% of the drivers and 11% of the pedestrians involved in accidents fall into that category. One must accept Durfee's comment that these figures are probably an underestimate by an unknown amount.

Heise (57) tried to define "intoxicated" in terms of an objective measure. He found alcohol in the urine up to 0.02% after 30 cc. of whiskey, alcohol up to 0.10% after 150 cc. Although the subjects on the smaller dose showed loss of efficiency in typewriting, and those on the higher dosage performed routine driving tests adequately, he argues from the loss of nonhabitual reactions on the larger dose that the line between "normal" and "intoxicated" should be determined by the presence of more than 0.02% of alcohol in the urine. In view of his evidence this would seem to be an arbitrarily chosen point; there would be argument in favor of placing the limit either higher or lower than the one specified.

Vernon (120) has also reported a study of driving ability under conditions simulating those of actual driving, with a mechanical record of deviations. Twenty subjects were given whiskey in doses varying from 30 to 45 cc., according to the weight and susceptibility of the subject. Learning effects made it impossible to run a continuous series of records with graduated doses. The conclusions from the data seem to be unsupported by the nature of the statistics; he concluded that there is great speeding of driving time and increase of errors after alcohol.

Holcomb (60) obtained blood samples from 270 drivers involved in personal injury accidents and from 1750 other drivers selected at random from the same area. He found that the percentage of drinking drivers involved in personal injury accidents varies as does the percentage of drinking drivers. "As the concentration of alcohol in the blood increased, the number of drinking drivers in the personal injury accident group greatly exceeds that in the general driving population." He places the line of nonsignificance at 0.5 parts of alcohol in 1000 parts of blood.

Paproth (96) questioned the assumption that there is an increase in industrial accidents on Monday, or that, if there is, it would necessarily be due to a larger week-end consumption of alcohol. Approaching the question from another angle, Vernon (121) showed that chronic drinkers sustain nearly three times as many accidents per 1000 as does the general population and lose more than three times as many days work as a result of the accidents. His figures are based on 39,793 men over 18 years of record.

From a survey of criminal statistics in Scotland over a 15-year period, Ross (99) showed that, while there has been a 50% decrease in offenses against the liquor laws during that period, the curves of crimes against persons and crimes against property have re-

mained practically straight lines. This would suggest that alcohol is not a major factor in causing these crimes. Benjamin and Fränkel (10) criticize all police record statistics on the ground that there is a socially selective factor in arrests which makes any conclusions from such statistics untenable. Adler (4) feels that there may be a real, although indirect, relationship between alcohol and crime, in that anything which upsets the balance between the human being and his environment may produce situations from which a criminal act may result.

In view of these inconclusive opinions and indefinite evidence on the effects of alcohol on accidents and crime, one should perhaps be surprised to note that Levi Bianchini (69), in Italy, and Flaig (43), in Germany, have both written condemning the alcoholic as a socially dangerous person who should be sterilized. However, the general trend of the literature on *treatment* is toward the psychological and psychotherapeutic approach. Persuasion during hypnosis has been successful for Kallenberg (64) and Schultz (103). Juliusburger (63) recommends a re-evaluation of social values and new goals for life. He does not give details as to the methods whereby this is to be brought about. Chambers (27) seeks to re-educate his patients to face reality, and Menninger (83) feels that there should be a long and intensive therapy, during which the patient must be helped to gain insight and to reconstruct his personality. Kantorovich (65) has been successful with 20 patients whom he reconditioned against alcohol by means of a strong electrodermal stimulus.

COMMENT

This multiple attack on the problem of alcoholism would have been far more productive of information if there had been greater uniformity of approach. The tendency has been for each investigator to set up a single problem, with too little reference to previous experience of other investigators. Animal experimentation has used a wide range of dosage and a great variety of technique; there has been a tendency to perpetuate errors of interpretation (as in the question of sex-cell damage). In the field of human experimentation the number of cases in any given study is so small that no adequate conclusions can be drawn; frequently the study is presented as a "preliminary investigation," but for the most part subsequent and better controlled studies have not been forthcoming.

There has been great need for some sort of arrangement among investigators in this field whereby agreement could be reached as to the range of dosage to be studied, types of administration to be used, and a division of problems so that duplication of inadequately controlled investigations might be avoided. Also, a better understanding of statistical principles would prevent the publication of studies based on cases so few in number that the conclusions can have no reliability.

An associated society of the American Association for the Advancement of Science, incorporated in 1938 as the Research Council on Problems of Alcohol, has been set up to meet this need. The Council has recently issued the first number of its Journal (61), in which studies by members will be published. This first number is in the nature of a symposium on the present status of the various facets of the problem. The list of contributors and editors includes most of the names best known in the study of alcohol at the present time. It is to be hoped that the Council will be able to maintain the Journal as more than a convenient outlet for publication of articles on alcohol. The first issue contains a review of the literature for 1939 (61) and promises an annual review if interest warrants.

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RECENT STUDIES OF THE LEVEL OF ASPIRATION

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The individual's evaluation of himself is generally agreed to play a major role in personality and motivation. A promising experimental approach to certain personal, environmental, and social factors influencing evaluation of one's own ability appears to be afforded by recent studies of the level of aspiration.

DEFINITIONS

Discussion of these experiments requires definitions of the terms *level of aspiration*, *success*, and *failure*. The original study (**19**) defined the level of aspiration as the totality of goal settings, and success and failure as subjective experiences. All later investigators have adhered to strictly behavioral definitions of these concepts. For them, the *level of aspiration* represents the *level of future performance in a familiar task which an individual explicitly undertakes to reach* (**7, 10, 13**). *Success* and *failure*, similarly, have referred to performances defined in terms of (a) their relation to the level of aspiration—a success being a performance above it, a failure one below it (**1**); (b) the adequacy of the performance to the task, often supplemented by reference to the performance of the group—a success being a good performance, a failure a poor one by these criteria (**4, 24, 26**).

The results obtained in the first and later studies have been entirely compatible, suggesting that these differences in definition are more apparent than real. The behavioral definitions seem more satisfactory for experimental purposes and will be adhered to in the following exposition (**12**).

PROCEDURES

These investigations have been confined to German and American children and university students, and certain types of psychotics. The original investigation (**19**), using a variety of tasks, studied the condition of success and failure experiences as inferred from spontaneous utterances and general behavior. All later investigators have used only simple tasks with a unidimensional

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quantitative scale of difficulty, namely: a series of similar tasks (mazes or pegboards) graded as to complexity (**4, 20**) or simple repetitive tasks with an achievement scale of speed or accuracy (**7, 10, 13**). The subject indicates his level of aspiration with the first type of task by choosing a task of a given degree of complexity, with the second by stating verbally or in writing the point on the achievement scale he intends to reach. The level of performance in the first type of task is defined only as accomplishment or nonaccomplishment at a given level of complexity. In the second type it is expressed in terms of the same scale as the level of aspiration, and the subject may also be told the relation of his performance to the average of his own or other groups (**2, 11, 16**). With all material the apparent level of performance may be modified without the subject's knowledge through using apparently soluble, but actually insoluble, mazes (**4, 20**), through falsifying the time scale (**6**), or through expressing the level of performance to the subject solely in terms of a nonverifiable relationship to the group average (**10**).

In all studies the subject is given some familiarity with the material before being asked to indicate his level of aspiration, usually through practice trials, occasionally simply through statements about the task (**3**).

Scoring

Several quantitative measures of the level of aspiration yielded by these techniques have been used. (a) *Height* is expressed directly in terms of the achievement scale (**3, 4, 6, 20**) or, more commonly, as the average difference between a series of levels of aspiration and immediately preceding levels of performance, the *average difference score* (**7**). Transmutation of these scores into standard scores permits their comparison in tasks with different achievement scales (**10, 13**). (b) *Rigidity* or, conversely, *mobility* is determined either by the relation of the number of shifts to the total number of levels of aspiration (**7, 26**) or by the sum of the sizes of the shifts (**4**). (c) A related measure, *responsiveness*, is given by the number of times the level of aspiration moves in the same direction as the preceding performance (**1, 4, 26**).

Other interesting measurable aspects of behavior in the level of aspiration setup are the time required to choose a given level of aspiration, and the number of voluntary choices, when the subject is permitted to break off at will (**4, 20**).

RESULTS

Reliability and Generality. These criteria have been applied only to the average difference score, which several investigators find to be highly consistent in a single task. In several tasks two independent observers have obtained split-half correlations of not less than .95 (**10, 13**). Reliability remains high despite an interval of a week between sessions (**7**).

Correlations between average difference scores in different tasks range from .25 (**13**) to .70 (**10**), all but two of the 24 reported being statistically significant. Such correlations are influenced not only by underlying personality characteristics, but probably also by such situational factors as phenomenal similarity between the tasks (**7**), levels of performance, and whether the tasks are in the same or different experimental sessions (**13**).

Statistical studies of this type are clearly too superficial to cast much light on the dynamics of the level of aspiration. Their chief value lies in demonstrating that this behavior is sufficiently stable and consistent to justify its being singled out for study.

Level of Aspiration vs. *Judgment.* The level of aspiration represents to some extent an objective judgment of probable future performance. That this need not be the sole factor operating has been shown by two studies comparing the individual's prediction of his own future performance (his level of aspiration) with his prediction of someone else's (an objective judgment) (**9, 22**). Despite differences in technique, both studies showed that the level of aspiration as compared with a judgment tended towards a somewhat higher and more variable difference score and towards markedly greater rigidity.

Effect of Properties of the Task. The behavior of the level of aspiration is obviously partially determined by such structural properties of the task as the number of steps in the achievement scale, the degree to which individual trials are emphasized, and whether or not the subject is forced to change the height of his level of aspiration after each trial. The apparent difficulty of the material is particularly important. If the task appears much too easy or much too hard, the dynamics of the situation are entirely different than if the task lies within an intermediate range of difficulty (**19**). Moreover, subjects in a competitive situation tend to overestimate their probable progress when the task is made progressively harder without their knowledge, and to underestimate it if the task is made progressively easier (**25, 28**).

Effect of Success and Failure in the Same Task. Any single level of performance may lead to a rising, falling, or unchanged level of aspiration depending on the momentary constellation of situational and personal factors (4, 13, 20). In general, the level of aspiration tends to follow the level of performance, but responds more readily to success than to failures. This is demonstrated by the greater number of upward shifts after success than downward shifts after failure (4, 20) and by the fact that the average difference score is usually positive (8, 9, 10, 13, 22). Furthermore, the average difference score tends to be more positive in a series of failures than in a series of successes. Thus, the average difference score is greater in a falling than in a rising part of the performance curve (10). Anderson and Brandt (2), in a study of American school children, showed that children whose performance in the experimental task was in the lower quartile of the class had a highly positive average difference score. Those in the upper quartile had a negative average difference score. Sears (26) found that children who had done poorly in the experimental task, or in a school task similar to it, had a higher average difference score than those who had done well.

In the above three studies, the ranking of the subject's performance with respect to the group was stressed to him. That this relationship was probably an important determinant of the results is suggested by the findings that knowledge of the supposed average performance of a group tends to raise the levels of aspiration of those whose performances are below it, but does not affect the levels of aspiration of those whose performances are above it. Furthermore, the upward pull is more marked when the group is "inferior" (unselected W.P.A. workers) than when it is one's own (16). Chapman and Volkmann (3) found, on the other hand, that knowledge of the performance of others has no effect on the height of the level of aspiration if the subject knows his own past level of performance in the task. The discrepancy probably arises from differences in subjects and technique, causing the level of aspiration to be more influenced by dynamic factors in the former study, more by perceptual "anchoring points" in the latter.

Effect of Success and Failure in Other Tasks. The degree to which successes or failures in one task affect the first level of aspiration in another appears to depend primarily on the perceptual similarity of the two tasks (6, 20). Later levels of aspiration in the second task seem to be influenced by broader situational factors,

such as the degree to which the subject regards his level of performance in both tasks as a measure of his worth (6).

Age. Studies of children's reactions to success and failure (5, 24) and to coöperative or competitive situations (17, 21) indicate that behavior suggesting the presence of a level of aspiration—for example, choosing to repeat an unfinished, in preference to a finished, task (24) or attempting to excel (17)—does not appear until the child has developed awareness of a "self" whose individual and social value is affected by his performance.

Sex. That sex differences in the behavior of the level of aspiration may warrant attention is suggested by incidental findings in two independent investigations that females show lower average difference scores than males (2, 9).

Personality Ratings and Tests. Many investigators have felt that the behavior of the level of aspiration might express more inclusive personality patterns. Thus, it has been suggested that self-confidence, ambition, subjectivity, wishfulness might find expression in a high level of aspiration; realism, cautiousness, self-protectiveness, fear of failure, "sensitivity to load" in a low one (1, 7, 18, 19, 20, 26). Attempts to verify such impressions through conventional statistical methods have been only partially successful. One investigator using statistically reliable ratings of certain personality attributes found suggestive, though statistically unreliable, relationships between high average difference scores and dissatisfaction with status (cf. 14) and between low average difference scores and fear of failure (11). Another found that children with the highest average difference scores were rated by others as more self-motivated than socially motivated, those with the lowest difference scores as highly socially reactive and less self-motivated (26). A group of college students showed suggestive positive correlations between the height of the average difference score and tests of subjectivity and between rigidity and tests of tenacity of purpose (23). Another study obtained only insignificant correlations between average difference scores and tests of dominance feeling and introversion-extraversion (15). The variability of these results is probably due rather to too superficial analysis of the problem than to a real absence of relation between personality attributes and the behavior of the level of aspiration (10, 13).

Psychoses. Two studies of the level of aspiration of psychotic subjects have been reported. The first, which was purely orientative, noted, among other observations, that rigidity of the level of aspiration tended to parallel rigidity of the personality in para-

noid reactions. Total lack of relation between level of aspiration and level of performance might accompany impairment of judgment in schizophrenic reactions (18). A particularly illuminating investigation showed that the level of aspiration of manics tended to show great mobility, with especial sensitivity to failure. The behavior of the level of aspiration of depressed patients tended to be governed more by increased sensitivity to social standards than by success and failure (4).

Attitudes. Certain transient attitudes evoked by the interplay of person and situation influence the level of aspiration. For example, both a self-competitive attitude and the regarding of the task as "play" seem to be accompanied by high average difference scores (8, 13, 26). Low average difference scores may accompany exaggerated sensitivity to the social aspects of the situation (8, 26).

Social and Cultural Factors. Full understanding of the behavior of the level of aspiration requires consideration of the influence of the social and cultural background. It has been found, for example, that the average discrepancy score tends to be higher in individuals with an inferior socioeconomic background than in those with a better one (14). A similar influence of the cultural milieu may account for the observation that in identical experimental situations Germans seem to show a much greater tendency than Americans to lower the level of aspiration after failure (4). A recent investigation has shown that an individual tends to shift his own average difference score in the direction of the supposed average difference score of his group (27).

DISCUSSION

The behavior of the level of aspiration is necessarily simple, due to restrictions imposed by the experimental conditions. Actually, it represents the final integration of complex and constantly shifting personal and situational factors. On the basis of the data at hand a few such factors may be singled out. The *level of aspiration situation* is usually a *threat to the subject's self-esteem* in that he must not only exhibit his ability before someone else, but must openly commit himself as to his expectation of future achievement. The subject attempts to meet this threat both by performing well and by manipulating his level of aspiration. Involvement of the subject's self-esteem may often be inferred from tension, obvious effort to do well, acute awareness of the experimenter, and other signs that he regards his own "worth" as involved.

Social and cultural factors are important in such a situation,

especially subject-experimenter and subject-group relationships and the "demands" of the cultural milieu as to both achievement and statements about achievement (13).

The *level of aspiration* usually represents a compromise between the subject's evaluation of his ability with respect to the difficulty of the task and his desire to achieve a high level of performance—that is, between a judgment and a goal.

As a *judgment* the level of aspiration ordinarily tends to remain close to the actual level of performance. This tendency probably arises from the almost universally present need to keep in touch with reality. Its relative strength may be increased by such factors as the degree of the subject's "detachment" or a belief that the experimenter wishes him to estimate his future performance as closely as possible (8). Occasionally it finds expression in a conscious intention to make level of performance and level of aspiration coincide (9, 13).

In so far as the level of aspiration is a judgment, it is largely determined by perceptual "anchoring points," of which the most influential is the subject's own past performance (3).

As a *goal* the level of aspiration tends to remain well above the level of performance in that it expresses the wishes to do well and to improve. High achievement and constant improvement, being socially approved, increase self-esteem.

Two other aspects of the level of aspiration may be briefly mentioned. It may be used to *improve performance* by being placed far enough above actual performance to act as an incentive. Conversely, if a high level of aspiration injures performance by making the subject tense, he may try to improve his achievement by lowering it. Secondly, the level of aspiration may be used to help *protect the ego from the effects of failure* by being kept resolutely high despite poor performance. This public refusal to admit that the failures are significant aids the subject to disregard them and also, being socially valued, strengthens him with respect to the experimenter. On the other hand, if the subject experiences a performance below his estimate as a severe threat to his self-esteem, he may keep his level of aspiration low to prevent such a situation arising.

It is apparent that the level of aspiration may have many different meanings. For example, a high level of aspiration may represent a direct expression of a goal, an incentive to better performance, or a means of protecting the ego. A low one may express an objective judgment, a method of avoiding tension, or a way of

avoiding the appearance of failure. Furthermore, successes and failures may not only influence the behavior of the level of aspiration in any one of its meanings, but may cause it to change from one meaning to another (13).

More important than the complexity of the factors underlying the level of aspiration is their amenability to experimental study. The significance of studies of the level of aspiration lies in their demonstration of a promising experimental approach to problems of success and failure, of the formation of goals, and of the genesis of the "self" and its relations to personality structure, achievement, and the social environment.

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THE LOGIC OF MODERN PSYCHOLOGY

I

PRATT, C. C. The logic of modern psychology. New York: Mac-
millan, 1939. Pp. xvi+185.

Psychologists are perennially interested in the general system-
atic problems of their science, and they have shown a vigorous
interest in the contemporary developments in the logic of science,
especially as represented in Operationism and logical positivism.
Professor Pratt has a fortunate combination of these interests
which has resulted in a valuable and provocative contribution to
the logical foundations of science in general and psychology in
particular. One may disagree with his position with respect to the
logic of science, or with some or many of the particular inferences
regarding systematic problems of psychology which he asserts to
be the logical consequents of his "critical positivism"; but one
cannot deny that the work as a whole is thoughtful and timely.

The exposition of the main thesis is lucid, although rarely ex-
haustive and sometimes unconvincing. As a prelude to the main
body of the work, Pratt insists that psychology does not have an
ontologically differentiated subject matter, the Mind. Mental and
physical phenomena cannot be distinguished operationally, and
the mind-body problem becomes, therefore, outside the range of
scientific inquiry. All sciences have the same subject matter,
which is the initial data of observation, and science must be monis-
tic. All sciences start with events and their relations, proceed to
abstract from these certain dimensions of descriptive differences,
and then formulate theories, or descriptions with greater general-
ity, which systematize the individual observations and dimensions
of description. It follows that psychology is merely one of the
family of sciences, the distinctions between sciences being based
on a division of labor. Similarly, all schools or points of view within
psychology have the same subject matter, and attempts to restrict
the scope of psychological inquiry to direct experience, or to intro-
spective, subjective, objective, or biological data, are operationally
meaningless.

The main thesis is presented in the last four chapters which are
entitled "Logical Empiricism (Operationism)," "Operationism in
Psychology," "Psychological Physiology," and "The Mystery of

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Mind." It proceeds from an acceptance of Logical Empiricism, or Operationism, through an evaluation of the functions that the operational point of view can perform for scientific psychology, to the conclusion that—on operational grounds—theory in psychology must of necessity be formulated in physiological terms. The last chapter, on "The Mystery of Mind," is in part a discussion of the nature of explanation in science and in part an elaboration of the plea for physiological theory which leads to the conclusions that (a) "for scientific psychology the descriptions of man's higher activities are relatively unimportant" (p. 167); (b) applied psychology and educational psychology are forms of charlatanry; and (c) psychologists should eschew all practical considerations in the formulation and direction of their research efforts. The organization of the argument suggests, therefore, that these, too, are logical deductions from the operational point of view *via* the first-order deduction that psychological theory should be attempted only when physiological constructs are possible.

These are startling conclusions, and they are sure to reorient the attitudes of both proponents and opponents of Operationism if they are valid deductions from that theory of science. The first critical question is whether Pratt employs the term "Operationism" in its accepted sense, and the answer is affirmative. The logical viewpoint is essentially that of the Vienna school of Logical Positivists, but Pratt prefers to call it "Critical Positivism" or "Operationism" because the emphasis is placed on the logical relations of observations and concepts rather than on the analysis of the formal properties of the language of science. Basic to the acceptance of this view is the proposition that "a fact is always a function of its method of observation . . . no scientific datum can ever be completely independent of the devices used to observe and report it" (pp. 62–63). Concepts are necessary devices for reporting and organizing observations, and the concepts of science, as distinguished from metaphysical concepts, must be tied to the conditions of observation of the event denoted.

Operationism is not, however, offered as a novel or final answer to the scientist's recurrent difficulty with his concepts. The completely operational definition is a zenith, of which the metaphysical definition is the nadir. It is rarely achieved in practice. Concepts usually transcend and fall short of the observations on which they are based because facts and verbal or mathematical concepts cannot be identical, the need for simplification gives concepts unwar-

ranted generality, and all interpretation of concepts is subject to the personal history of the individual scientist. Operationism merely demands great caution—says "be careful"—in the formulation and interpretation of concepts when it maintains that "the meaning of . . . (a concept) can only be discovered by finding out just what was *done* and what was *observed*, or, in other words, what *operations* were carried out before the concept came into use" (p. 73). Mathematical equations are the most nearly perfect form for the definition of concepts, and it is said that psychologists and biologists have erred more often than physicists in accepting non-operational definitions because they use equations less often.

Although Operationism is denied as a panacea for the ills of contemporary psychology, it is maintained that it may be used to clarify the problem of how to be careful in the definition and use of concepts. Its usefulness is further limited by the assertion that of the three important phases of scientific method—the collection of initial data of observation, the statement of inferences from observations in the form of laws, hypotheses, theories, concepts, and the deduction of consequences from laws and hypotheses—only the construction of laws, hypotheses, theories, and concepts will be aided by insistence upon the operational point of view. The usefulness of Operationism in the collecting of initial data of observation is categorically denied, apparently in the belief that if it is so used to direct the collection of data it is likely to be misused for the unwarranted exclusion of some types of data. Thus, Pratt rejects the identification—attributed to Tolman—of Operationism and behaviorism, and reaffirms his position that ontological differences in scientific subject matter do not exist. There follows a brilliant and thorough analysis of the several meanings of the terms "objective" and "subjective," which leads to the conclusion that the only question for science is whether events can be adequately reported and their conditions specified. The question of the objectivity or subjectivity of data is meaningless.

The clarity and force of this position is lessened somewhat by a peroration directed against S. S. Stevens' conclusion that Operationism redefines the problem of psychology and the basis of physics as the differential reaction, in place of the traditional immediate experience. Pratt objects that "colors, sounds, lights, odors, temperature, and the varieties of shapes and movements of all objects of the whole great globe itself: these all fade and dissolve, leaving nothing behind more substantial for science than a differential re-

action" (p. 103). It seems to the reviewer that Stevens is merely asserting in a different way that the subjective and objective cannot be differentiated operationally because the subjective (immediate experience) must be made objective (report) before it has scientific status. Pratt may even be accused of forgetting for the moment his dictum that "if modern psychology therefore presents an account of human nature which is at variance with that of common sense and intuition, the trouble is not with psychology, but with those whose adverse criticisms of modern psychology on this score betray such deep misunderstanding of the logic of science" (p. ix).

The student of experimental method in psychology may wonder whether Pratt has not permitted his concern with the problem of subjectivity-objectivity to blind him to certain real, and important, values of the operational point of view in directing the collection of observations. If concepts must be defined in terms of the operations and measurements employed in demonstrating the phenomena to which they refer, the one way in which complete specificity of those concepts may be avoided is through the standardization of certain basic conditions and methods and the calibration of variants in terms of these standards. Otherwise, the minor variations of technique employed by one investigator of the phenomenon make his concept unique. The absence of a bias toward standardization has retarded to a marked degree the progress of empirical generalization in special fields of psychology. It is to be hoped that one of the fruits of Operationism in psychology will be a greater emphasis on such standardization and calibration of operations.

One may also question Pratt's statement that Operationism has nothing to offer the scientist when he is at the point of deducing the consequences of laws and hypotheses. The discussion of the recent efforts of Hull to develop mathematico-deductive theories in psychology is concerned primarily with the question whether psychological data are yet sufficiently precise for such theoretical elaboration except in very restricted areas. It would have been revealing for Pratt to apply the operational criterion to the deductions stated by Hull in his "Psychologies of learning: a way out." To the reviewer it seems self-evident that a deduction cannot be given a conclusive experimental test unless the conditions, methods, and measures to be employed in the test are either explicitly stated or are implied by the homogeneities in the operations defining the

empirical or hypothetical concepts which have been used as the basis of the deduction. Examination of Hull's "postulates" reveals that some have been derived from conditioned-response experiments on the dog, some have been derived from nonsense-syllable learning in the human subject, and that the exact conditions of the experimental test of the deduction of "reminiscence" in rote serial learning cannot be specified. Should not such deductions be classed with the other operationally meaningless questions or statements which receive so much attention in Pratt's attack on verbal magic in psychology? In short, is it not possible to set up certain rules for the profitable application of the deductive method in theorizing which are based on the implications of the operational point of view?

Pratt would have made a more convincing case for the value of Operationism in psychology if he had developed these implications for the standardization of observational techniques and for deductive theorizing, because even in the construction of concepts, laws, and hypotheses, where he finds the only important values of Operationism, he finds a paradox for Operationism. On the one hand, insistence upon operational definition aids in avoiding the common errors of *reification* and the *infiltration* of theoretical and nonoperational meanings in concepts which began with operational definitions. On the other hand, science cannot, Pratt insists, be concerned solely, or even primarily, with observed correlations of events (material properties). It must, instead, concern itself with constructs (formal properties) which are at least one step removed from the observed events. That is, science must, as in the case of the concepts of the synapse and the Titchenerian sensation, transcend its operations in order to arrive at meaningful theoretical organizations of its data. All that Operationism can do is to "keep scientific imagination within palpable boundaries" (p. 128). This will be accomplished in psychology through the use of two guiding principles in theorizing: (1) "the firm insistence that the contours of the guesses must conform to the contours of the gaps revealed by the observed items" (p. 128), which means that the terms of all hypotheses should be merely the correlations of events, since such is all that science can ever encompass; (2) the guesses in psychology should be phrased in physiological terms because the material counterpart of any psychological construct must necessarily be found inside the organism. These two principles are, therefore, the main contributions of Operationism to psychology.

It seems to the reviewer that the first of these two principles is, in itself, sufficient reason for insistence upon the operational definition of psychological concepts, and it is unfortunate that Pratt found no better language in which to state the principle. The substance of the idea becomes clear in his extensive application of it in his criticism of the term "dynamic" as used by some psychologists and in his criticisms of the concepts of "type" and "trait," but the reader is likely to remain puzzled by the insistence that scientific imagination remain within *palpable* boundaries, and that the *contours* of the hypotheses conform to the *contours* of the gaps revealed by observed data.

The statement that the operational point of view leads to the conclusion that theory in psychology must be physiological should arouse a storm of opposition from avowed operationists who have been critical, on operational grounds, of the supposed dependence of psychology on physiology. Pratt's defense of this inference rests, first, on the argument that theory in psychology is concerned with the variables which intervene between the environment and behavior. These variables cannot be in the environment or in behavior itself, so they must be in the organism. The second argument comes more directly from the general operational thesis that it is " . . . mandatory upon psychologists to place (the immediate determinants of psychological events) within a definite region accessible to human observation and verification" (p. 129). Strangely enough, it is the inaccessibility of these very physiological data which has led Skinner, McGeoch, Tolman, and others to insist upon the development of psychological concepts which have definitions in terms of environmental conditions and behavior events rather than hypothetical physiological events.

Pratt answers in detail the points made by these antiphysiological operationists, but he fails to consider their positive suggestions or demonstrations of an independent science of behavior. He also mistakes their rejection of physiological theorizing for a general antitheoretical attitude which none would defend, and he mistakes their justifiable criticisms of specific neurological and physiological concepts for criticism of the right of a scientist to formulate physiological and neurological concepts which have some operational meaning aside from the specific set of observations they are designed to explain. Pratt rightfully maintains—although the emphasis may be misplaced—that all science is tautological and

circular, that nothing is true until it is known to be true, but it does not seem to the reviewer that this is an answer to McGeoch's statement that "to make the perseveration theory applicable (to the explanation of retroactive inhibition), one must endow it with all the properties implied by the results and in the absence of knowledge about perseveration this endowment leads at once to circularity." In this context the accusation of circularity means merely that the operations which define retroactive inhibition also define neural perseveration, and under such circumstances one gains nothing in the way of novel deductions or predictions from the substitution of words. If such physiologizing is not an instance of word magic, equal in meaninglessness to the explanation of behavior in terms of powers and purposes, Pratt fails to make the distinction clear.

In reality, the adherents of an independent psychology will admit that physiological interpretations, or correlations, of behavior phenomena are a legitimate field of inquiry. The whole matter seems to be, then, a matter of difference in emphasis on the heuristic value of physiological guesses in psychology. But Pratt is dogmatic on this point. He says, for example, that "theoretical psychology can advance only in proportion to its ability to frame its explanatory principles in the language of physiology" (p. 131), and this is held to be true even though the physiological concept is a formal construct which can be operationally defined only indirectly through the behavioral data from which it, like the concept of synapse, emerged.

In the final chapter, entitled "The Mystery of Mind," Pratt extends his observations on such matters as the relative unimportance of initial data and the importance of theory, the reduction of all explanation to more complete description, the notions of circularity and tautology in science, and then brings forth a distinction between causal and empirical laws in science which is the basis for an evaluation of the present status of scientific psychology. Causal laws are formulations of observed correlations to which some method of concomitant variations has been successfully applied. Empirical laws also represent formulations of observed correlations, but in their case some of the antecedent terms are not subject to control or variation, or some of the items which unquestionably influence the correlations are inaccessible to observation. The differences between the laws, *i.e.* with respect to

the control of the independent variables, the precision of measurements involved, and their predictive efficiencies, are admitted to be relative, not absolute.

It is said that the difficulties of manipulation of the variables involved in psychological events makes most of psychology a collection of empirical, rather than causal, laws. "The method of concomitant variations can thus be applied, with fair success, to problems in such fields as sensation, also reflex action, perception, learning and memory, emotion, to a few problems in intelligence and reasoning—in short, to problems in which some control of stimulus variables is possible, and in which the physiological state of the organism can be kept moderately constant" (pp. 159–160). But "the important determinants of man's higher activities are inside him, which means that to apply the method of concomitant variations to them is still out of the question" (p. 162). Although the definition of "higher activities" is not given, even by simple enumeration, they are, presumably, the personality characteristics, interests and attitudes, creative efforts, and loves and hates of the everyday man, and these data, according to Pratt, should be left "strictly where they belong—at the level of description" (p. 167). They have no importance for scientific psychology because they suggest no physiological theories of value.

Along with man's higher activities Pratt would like to disown such fields as applied psychology, on the ground that applications of psychology in the present immature stage of psychological theory is charlatanry. Thus, "no proof can be furnished that the science of mental healing, as practiced by psychoanalysts, is any better than Christian Science" (p. 169), and "leaders of men are not aware of any psychological laws, nor would they be helped by knowing the contents of all the textbooks on applied psychology now in existence" (p. 170), and "(interest, enthusiasm, and a pleasing personality) can be well enough ascertained after sufficient acquaintance by any person of average intelligence" (p. 175). There follows a general castigation of applications of psychology and a plea for a revival of science for science's sake, of a pure psychology of the laboratory as a prelude to later application.

These are strong statements, and they need much more defense than Pratt gives them. A multitude of questions come to mind, even though the reader is neither an applied nor an educational psychologist and has no stake in their prestige or success. Are there not data which have significance for the governing of human

affairs which have been obtained by the application of the method of concomitant variations? Are there not so-called pure psychologists who have committed unpardonable methodological and interpretational sins, just as there are applied psychologists and educational psychologists who have done so? What justification is there for tarring all psychoanalysts, all clinical psychologists, all educational psychologists with the same brush? What is Pratt's evidence that a person of average intelligence and no training in psychology can do as good a job of vocational selection and counseling as a trained clinical psychologist? What are these "higher mental processes" which Pratt is willing to strip from psychology as fields of investigation? Those were not only strong statements; they were devastating statements which, if presented in good faith as legitimate inferences from the logic of modern psychology, and not as mere personal prejudices, deserved much more complete documentation than Pratt gives them.

"Pure" psychology does not escape from this wholesale flagellation. There are many references to the poor state of psychology, frequently in the form of asides, of which the following are representative.

In psychology the possibility of accurate prediction is so rare that the test (of the validity of a concept) has small value (p. 39).

Altogether too much time, in psychology at any rate, is devoted to "research" of the dictionary sort (p. 72).

It takes psychologists to be impressed by such hackneyed stuff . . . (p. 101).

The animistic tendency runs riot in psychology, and seems to show no signs of abatement (p. 122).

The psychological fields to which the method of concomitant variations can be applied with some success are few in number . . . (p. 159).

And these are capped by the famous quotation from James—with the implication that it is as true in 1939 as it was in 1892—on the status of psychology:

A string of raw facts; a little gossip and wrangle about opinions; a little classification and generalization on the mere descriptive level; a strong prejudice that we have states of mind, and that our brain conditions them. . . . This is no science, it is only the hope of a science (p. 169).

These are inferences from Pratt's own nonscientific biases and frustrated hopes for a super-rapid development of experimental psychology, not from a straightforward comparison of the psychological journals of today and yesterday or of the status of

knowledge in special psychological fields today and yesterday. Psychologists must deplore such loose generalization in an otherwise stimulating and consistent approach to the logic of modern scientific psychology.

<div align="right">

ARTHUR W. MELTON.
</div>

University of Missouri.

II

A REPLY TO PROFESSOR MELTON

BY CARROLL C. PRATT
Rutgers University

In controversial matters, such as theories of psychology, or international policies, I am temperamentally unable to remain fixed and convinced in my views over any considerable period of time. What Professor Melton read so patiently and sincerely in *The logic of modern psychology* was my somewhat stronger set of Tuesday, Thursday, Saturday, and Sunday convictions. I am nearly, but not quite, persuaded, in response to Professor McGeoch's generous invitation, to confine this reply to a confession that my weaker self of Monday, Wednesday, and Friday agrees with almost everything Mr. Melton said in his penetrating review.

Of the many provocative criticisms which Melton has directed against my views I can take time and space to answer only three: (1) the differential reaction as subject matter of psychology, or my lapse of memory on page 103 for what I said on page ix, (2) the question of physiological psychology, and (3) my attitude towards applied psychology.

(1) The scientific portrait of human and animal nature cannot resemble that of common sense, no matter how faithful the latter may be to the original, for the simple reason that they represent different levels of testimony. Psychology and common sense both start at the same place, but psychology as science, in contrast to common sense, goes far beyond its points of origin into realms of logical construction. It must always be possible to define concepts at this level in terms of the initial data of accurate common sense, otherwise their logic is no better than verbal magic; but the concepts and the initial data do not look alike. Concepts are generalized statements of uniform functional relationships; initial data cover the whole welter of things related.

Psychology deals initially with bodily movements, perceived colors and sounds, manics and introverts, the attitudes of voters and the fears of children, with the varieties and shapes and postures of nearly all objects of the whole great globe. No matter how elaborate and general or bizarre the subsequent formulations of theory may become, it must always be possible to retrace these formulations back to the things with which they started. When, therefore, on one page of the text I refer to the kinds of objects with which psychology deals (p. 103), and on another page I cite the disparity between the appearance of these objects and the theories of science devised to explain their behavior (p. ix), the difference in the two passages is not due to any lapse of memory on my part. Nearly the whole burden of the intervening pages was an attempt to show why such disparity is both justifiable and unavoidable in scientific work. Inconsistent as I frequently am, it was a matter of agreeable surprise to discover on rereading myself that in this instance my mnemonic traces revealed neither positive nor negative time-errors. My peroration with Stevens, as Melton calls it, did not concern this question of disparity at all.

There is no disagreement between Stevens and me—at least I do not think there is—about the difference between common-sense intuitions and scientific constructions. My only quarrel with Stevens concerns the operational significance of the differential reaction. Stevens argues that the initial data of description are identical with elementary reactions and that the sign of a thing is therefore the same as the thing itself. Finger reactions are tonal densities, scratches on a smoked drum are the feeding behavior of rats, stopping a car at a signal light is the color red, ability to note differences in bodily shapes is endomorphy, ectomorphy, and mesomorphy. I cannot here go over again the arguments which make it difficult for me to accept this view of things.

(2) With respect to physiological psychology I could wish that Melton had not overlooked my statement of belief that the position advocated by the strict correlationists, such as Skinner and McGeoch, is eminently defensible; that from the point of view of the logic of science their views represent, in the last analysis, the only tenable position (p. 133). The trouble in adhering strictly to their position is that psychology is not yet anywhere near "the last analysis."

Let us call the stimuli which impinge upon a biological organism A and the sensory and behavioral processes of an organism

B. At present the majority of psychological correlations are those which have been observed to obtain between *A* and *B*. The supporters of an independent science of psychology insist that the *only* correlations with which psychologists should concern themselves are those between *A* and *B*. I must confess that my own personal and professional interests and competence do not go much beyond these correlations, but at the same time I find myself compelled to recognize that such correlations are incomplete. Between the regions *A* and *B* there is a whole network of conditions, *C*, the biological processes inside the organism, upon which the correlations between *A* and *B* depend. To hold out for an independent science of psychology in the face of such inescapable dependency strikes me as wilfully stubborn myopia. Melton speaks of my inferences about the supposed dependency of psychology on physiology. Inferences and supposition indeed! Changes in sugar content of the blood, to cite only one of countless examples, produce marked alterations in nervous function and in mental states, especially among diabetics. And that's a fact.

To a certain extent I can agree with Melton that this whole matter is "a difference in emphasis on the heuristic value of physiological guesses in psychology." But then I do not understand, if this is true, why Melton also says that "if such physiologizing is not an instance of word magic, equal in meaninglessness to the explanation of behavior in terms of powers and purposes, Pratt fails to make the distinction clear." Let me try to explain.

In the first place, the distinction is indeed partly heuristic. If any given physiological hypothesis stimulates further research, as Sherrington's writings about the synapse certainly have done, the hypothesis has rendered valuable service regardless of whether it is true or not. The fact that similar research might conceivably have been undertaken without the stimulus of a shot in the physiological dark is beside the point. We are talking heuristics. If some psychologists are stimulated more by brain mythology than they are by the purely logical implications of cool correlations, it would be heuristically unwise to deprive them of such stimulation.

In the second place, physiologizing need never be verbal magic if it is constructed and used properly. If neural perseveration and retroactive inhibition are defined by the same operations, the two sets of words are logically interchangeable, and the meaning of one is just as clear as that of the other. Verbal magic creeps in only when someone deludes himself into supposing that neural persev-

eration *explains* retroactive inhibition. But if there is no explana-
tory value, why talk about neural perseveration? The answer is
twofold. One reason is heuristic, as mentioned above. The other
reason is psychophysiological. If a concept fits into a known body
of biological fact, as, unfortunately, neural perseveration does not,
it stands a better chance of independent verification. (Instead of
neural perseveration, take astigmatism and asymmetry of curva-
ture of cornea and lens, or the blind spot and the disappearance of
marks on a paper.) Independent verification offers a true explana-
tion or, better stated, a new and more far-reaching correlation. A
concept which had its origin in one system of correlations acquires
by virtue of its verification in another system of facts a whole new
set of implications belonging to that system. Psychological laws
will eventually be forced to include physiological principles. I feel
reasonably sure that the two disciplines will one day be indistin-
guishable. The sooner that day arrives, the better it will be for
psychology.

(3) Professor Melton is unnecessarily alarmed at my attitude
towards applied psychology, and his fears cause him to make accu-
sations which are more sweeping than my own intentional exag-
gerations. I certainly never meant to convey to the reader the
impression that I regard applied psychology as so much charla-
tanry. It seemed obvious to me as I wrote that I was barking worse
than I ever intended to bite. But in order that the reader might
not be misled into thinking that I was about to sink my teeth into
someone, I carefully warned him that "all of the above statements
and illustrations with respect to applied psychology are inten-
tionally a bit extreme—the sort of exaggeration a person indulges
in who knows there is a good deal to be said in favor of the point
of view he is trying to attack. But the exaggeration will do no
harm, for there are certainly more psychologists in this country
who are willing, in response to some query, to reply, 'Psychology
has this or that to say on such and such a subject,' than there are
who reply, 'Psychology has little or nothing to say' " (p. 175).

To point out the practical limitations of a science is not to
brand the discipline as charlatanry. I should not say that the fact
that weather forecasters are wrong about 15% of the time makes
them quacks. Nor do I urge psychologists to eschew all practical
considerations. "Where the need is great, where it is better to do
something than to do nothing at all, as in the case of mental dis-
orders, it may be justifiable to make use, with all possible precau-

tions, of whatever psychological knowledge is available" (p. x). Does psychoanalytic knowledge help mental disorders? Evidence is uncommonly hard to find and still harder to evaluate. Kessel and Hyman (*J. Amer. med. Ass.*, 1933) report 85% failure to effect genuine cure. It is obvious that a test of meteorological predictions is more accessible than any test as to whether psychoanalytic therapeutics have succeeded, so I yielded to the temptation in the text, as Melton quite rightly suspected, to express my personal doubts and hopes rather than search for questionable evidence.

It is more important now than ever before that moral and financial support be given to the pursuit of science for the sake of science, as well as for the sake of immediate and practical service. The time may not be far distant when a very considerable number of American psychologists will be asked to turn their thought and energy to problems connected with war. In some countries practically nothing but military psychology is allowed. However necessary and important such efforts may be in the present sad plight of world affairs, we should not suffer the habits of adversity and need to remain dominant in our science when the dawn of another day dispels the blackout. It is even conceivable that a wider love of disinterested truth may all unwittingly provide mankind with that practical knowledge required to beat swords into plowshares. I do not say that applied psychology is not important, but I do say that the history of science ought to remind us that a psychology devoted to truth for truth's sake may turn out in the long run to be more useful.

I should like to close with a sentence or two from my preface.

I know that this book will offend many psychologists. But I rather enjoy that prospect, for if the book helps to renew an interest in the basic assumptions of psychology, it will have had some small share in expanding what strikes me as the most important, but by no means the most vigorous part of psychology, at least in this country, namely, Theoretical Psychology (p. viii).

BOOK REVIEWS

THOMSON, G. H. The factorial analysis of human ability. Boston: Houghton Mifflin, 1939. Pp. xv +326.

Tracing his interest in factorial analysis, Dr. Thomson mentions his work as test-author, consultant, and examiner in endeavoring to improve the methods of selecting pupils for secondary schools. In the preface he says:

It was inevitable that I should be led to inquire into the use of intelligence tests for this purpose, and inevitable in due course that the possibilities of factorial analysis should also come under consideration. It seemed to me that before any practical use could be made of factorial analysis a very thoroughgoing examination of its mathematical foundations was necessary. The present book is my attempt at this, and as I wish to reach as many workers in this field as possible I have kept the *formulae* of mathematics out of it as far as I could. It may seem remote from school problems. But much mathematical study and many calculations have to precede every improvement in engineering, and it will not be otherwise in the future with the social as well as with the physical sciences (p. xv).

Professor Thomson has performed a very valuable service to psychologists and educational workers in so successfully accomplishing this purpose of describing a complex mathematical method of analysis in simple language. The higher mathematics which has so frequently discouraged persons wishing to learn about factorial analysis has been relegated to a mathematical appendix. The principal method of exposition is the use of an adequate number of very simple, worked-out examples. Whereas these very clear illustrations of computational methods should do much to inspire enthusiasm and confidence in factorial analysis, the skeptical attitude of the author should have a wholesome effect in preventing these new converts from taking their findings too seriously.

Since entering the field of factor theory more than twenty years ago as one of its most active critics, Professor Thomson has quite consistently been aligned on the side of the "doubters." Therefore, although the impression is gained that the author is making a definite effort in this book to be optimistic about the usefulness and value of factorial methods, it is probably not surprising that the reader is left with the impression that the author cannot quite convince himself that there really is much reason to expect anything from these procedures. This attitude is revealed in the last paragraph of the main part of the book:

Yet with all the dangers and imperfections which attend it, it is probable that the factor theory will go on, and will serve to advance the science of psychology. For one thing, it is far too interesting to cease to have students and adherents. There is a strong natural desire in mankind to imagine or create, and to name, forces and powers behind the façade of what is observed, nor can any exception be taken to this if the hypotheses which emerge explain the phenomena as far as they go, and are a guide to further inquiry. That the factor theory has been a

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guide and a spur to many investigators cannot be denied, and it is probably here that it finds its chief justification (p. 284).

In general, it may be said that the author has given his principal attention to those aspects of factor theory which are most closely related to his own numerous and valuable critical contributions. Since much of his work has been concerned with the fundamental problems involved in Spearman's theory of two factors, it is not surprising to find a very large portion of the book devoted to the discussion of the problem of a single general factor and Thurstone's extension of this problem to the multiple factor situation. A rough analysis indicates that approximately 75% of the pages are devoted to these topics, and another 10% are concerned with Thomson's own interpretation of the causes of hierarchy in correlation matrices. Inverted factor analysis, which involves the correlation between persons rather than tests, is given 10%, and in the remaining 5% a brief discussion of the Kelley and Hotelling methods of principal components is presented.

In Part I the fundamental problem of factorial analysis—obtaining a valid and useful explanation of the performance of individuals on various tests—is presented along with brief descriptions of the more widely used methods which have been proposed for the solution of this problem. The psychologist's difficulty is not in finding *a* set of theoretical factors or tests which will explain the relations between the test scores of the individuals which are shown by their correlations, but, rather, in *selecting* from the infinite number of possible sets of factors the one which will be of most assistance to him in accomplishing his particular purpose.

Part II concerns the estimation of individual scores in the new factors derived from such analyses as those of Spearman and Thurstone. It also, incidentally, provides a good discussion of the use of Aitken's method of pivotal condensation in obtaining regression coefficients and multiple correlation coefficients.

Part III discusses the difficulties introduced into practical problems because of always having to deal only with a rather limited and not truly representative sample. Much of the material presented here is relatively new and of considerable theoretical importance.

Part IV provides a very welcome critical discussion of recent work in obtaining and analyzing correlations between persons by comparing the two individuals' scores in a series of tests. A discussion of the relation between such "person factors" and "test factors" obtained from the same data is also given.

The topic which many regard as the most crucial in this field, the interpretation of factors, is discussed in Part V. This section should be required reading for all who venture into the realm of factorial analysis. Persons who are looking for someone who will give them the answers to the many difficult problems in this field will probably be disappointed. What they are given is, however, probably the next best thing, a good discussion of the limitations of certain procedures in current use. For example, the author concludes after a discussion of the implications of Thurstone's criteria of simple structure: "In general, simple structure will be

attainable with a battery of tests only when the battery has been picked with that end in view" (p. 265).

The Mathematical Appendix contains the mathematical statements and derivations relevant to the main body of the text, but carefully excluded from it. These are given in concise form and will provide a convenient reference for teachers and workers in this field.

In general, it may be said that the discussion of the various factorial methods shows relatively little partiality or prejudice. In comparing the Thurstone and Hotelling methods, Professor Thomson comments, "the system we prefer will depend largely on our motives, whether we have a practical end in view or are urged by theoretical curiosity" (p. 134).

This reviewer has been pleased to find so little with which to quarrel in this book. The typography is excellent, and it is most commendably free from typographical errors. The principal difference of opinion between himself and the author concerns the general point of the possible usefulness of factorial methods. He would, for example, object to Professor Thomson's statement that "the chief practical incentive is the hope that factors will somehow enable better vocational and educational predictions to be made. *Mathematically, however, as we have seen, this is impossible*" (p. 134). What was shown mathematically was that no better predictions can be made from a set of factor scores than from the test scores from which they were derived. However, it is the belief of the reviewer that with the assistance of factorial methods it will be possible to develop simpler and more unitary tests which will enable much more accurate predictions than the present crude and complex measures.

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REED, H. B. Psychology and teaching of secondary-school subjects. New York: Prentice-Hall, 1939. Pp. xviii + 684.

Books on the psychology of school subjects made their first appearance some twenty-five years ago. In 1916 Professor Frank N. Freeman wrote a book dealing with the psychology of the common branches, and in 1915 Charles H. Judd brought out a volume on the psychology of high school subjects. Since that time numerous books have appeared which treat the psychology of a single subject, as well as several books which cover the entire curriculum of the elementary school or the high school. The most recent addition to the latter class is Professor Reed's volume on the *Psychology and teaching of secondary-school subjects.*

In his preface Professor Reed announces his intentions to limit the scope of the book to a treatment of scientific investigations in the psychology and methods of high school subjects. The number of studies which purport to fall under this classification is obviously very large, and consequently the value of his contribution will be determined in no small measure by the selection of studies which he includes. It is, of course, impossible to draw any sharp lines to separate significant and trivial studies, and the author has tended in the direction of a liberal rather than a too narrow inclusion of data.

A general pattern of treatment has been followed for most of the sec-

ondary school subjects. The author attempts to review, organize, and interpret the selected psychological studies and also to point out their practical applications for teaching in secondary schools. In addition to this broad aim, he has developed his treatment around four fundamental principles of learning, namely: (a) organization or perception of meaning, (b) practice, (c) adjustment to individual differences, and (d) motivation. The treatment of each school subject is preceded by a somewhat extended statement of the objectives of teaching it.

In view of the importance in the author's treatment of the four principles just mentioned, some clarification in his own words may be in order.

Organization is the principle by virtue of which facts fall into a pattern and give meaning to the learner. The ease of learning depends upon the degree to which the learner perceives this pattern or upon the extent to which the facts studied have meaning. The principle of organization, therefore, emphasizes the study of relationships—the relations which give unity, coherence, and system to the facts or objects studied. The principle of practice means that improvement in learning is made by repeated efforts to do better. There are certain conditions which must be observed to make this possible: the learner must know the goal that he is trying to reach; he must have some method for reaching it; he must know the results of his efforts; and he must be able to evaluate them with reference to his goal. The principle of individual differences refers to the fact that an individual differs or deviates to a certain extent from the average of his groups, and that methods of learning which are to be effective for him must be modified so as to take due account of this deviation. Among the important differences to be considered in learning are differences in capacity, interest, and needs. The principle of motivation is interpreted to mean that students must have a motive in order to learn. A motive may be thought of as a need felt by the individual which creates a dominant tendency or activity directed toward an object by which the need is satisfied. In animals such needs are mostly organic; in humans they are not only organic, but also emotional and ideational.

The very large number of investigations covered necessitates brief treatment, but they are summarized clearly and with fairness. The problem of integrating and interpreting these studies is a difficult one, and the treatment here is not particularly outstanding. The volume would probably be more useful as a handbook for those who wish to follow up the original investigation than as a satisfying interpretation of the present psychological literature relating to secondary school teaching.

In the reviewer's opinion, the principal criticism of the book grows out of the author's difficulty in selecting studies which make a definite psychological contribution. The difficulty of selection may be due to an absence of predefined criteria as to just what ought to be considered "psychological" studies. Consequently, fundamental psychological research is treated along with more or less unimportant studies of method, and the outcome is somewhat confusing. The book may be more widely used as a text because it covers both psychology and methods, but the reader will have some difficulty in separating studies which contribute basic psychological data from studies which apply them to problems of

methodology. It would add to the clarity of the book if the psychological data and principles underlying methodology were first built up so as to afford a clear basis for their application.

Professor Reed has followed the pattern of a number of the later books on the psychology of school subjects, namely: to give the main portion of the book to descriptive reviews of psychological investigation and only a small portion of it to the author's own contribution. Where so much space is taken for description of studies there is scarcely any other alternative. On the other hand, a greater dependence on footnote references to be followed up by the reader would leave space enough for the integration and critical interpretation necessary if the psychology of school subjects is to make any real contribution to teaching.

Professor Reed has written a useful book which will probably be much used. On the other hand, it has not gone beyond previous books on the psychology of school subjects in making any important contribution to educational psychology. This shortcoming seems to the reviewer not to be due to any failure to treat carefully the scientific investigations in this area, but rather to a failure to interpret the investigations in terms of available psychological theory and research outside the narrow field of psychology of school subjects.

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University of Chicago.

GRAY, C. T., & VOTAW, D. F. Statistics applied to education and psychology. New York: Ronald Press, 1939. Pp. xiv +278.

A textbook in statistics written for the classroom teacher, the school principal, the student of psychology or education, and the educational research worker can hardly be expected to meet the needs equally well for all of these groups. For many years classroom teachers and school administrators in their training programs have been taught elementary statistics either in separate courses or as part of their work in tests and measurements. The effectiveness of that instruction can be summarized in collegiate parlance with the phrase: "It just didn't take." Teachers in their professional training learned the formulas and how to calculate measures of central tendency, measures of variability, and coefficients of correlation. They were not taught, nor did they learn, for the most part, what needs they would have for such measures or how to interpret and use them. Not having learned the needs, they soon found that in the classroom they could get along very well without statistics. When, therefore, one picks up a new book in this field and finds that it is for the classroom teacher, new hopes arise. Usually, as in the case of the Gray and Votaw book, those hopes vanish upon reading it.

The organization of such textbooks has become quite standard. Gray and Votaw do not depart from the usual form. They start with a chapter on making data meaningful, which includes a consideration of problems of ranking, tabulating data, determining the number and size of class intervals, frequency tables, and simple graphs. Chapter 2 deals with the arithmetic mean while other measures of central tendency are treated in Chapter 3. In successive chapters there follow treatments of percentiles.

measures of variability, normal probability curve and reliability, further applications of the probability curve, correlation, interpretations and uses of correlation, and other types of correlation.

The formulas and directions for calculating measures are clearly and simply presented. One illustration, however, will serve to show how inadequately the reader is prepared for interpreting these measures. In the chapter on "Interpretations and Uses of Correlation," the following help is given for interpreting a coefficient of alienation:

> By utilizing Formulas (51) and (52) the low efficiency of prediction possessed by apparently substantial correlations may be seen readily. For example, let us test a correlation of .70 (or −.70) in this respect:
>
> $$C.A. = \sqrt{1 - 70^2} \text{ or } .714$$
> Then $\qquad$ $E.P. = 100(1.000 - .714)$ or 28.6

The efficiency of prediction for a correlation of +.90 (or −.90) is only about 58, so it may be seen that improvement in efficiency of prediction as correlation changes from .90 to 1.00 is very rapid. Of course, the relationship of the variables C.A. and r in Formula (51) may be shown graphically for ready conversion from the one to the other.

Presumably this is written to help the classroom teacher or school principal interpret a coefficient of alienation!

The student in education or psychology and the educational research worker will find this book about as useful as any one of the better five or six standard elementary texts on the market. Some day a book will be written that begins with the problems of the classroom teacher and/or school principal and shows how statistics can be used in their solution. This the Gray and Votaw text does not do.

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Stanford University.

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NOTES AND NEWS

DR. CLARENCE LEUBA, chairman of the department of psychology at Antioch College, is acting as guest professor of psychology at the University of Indiana during the current semester.

A short bibliography on the psychological aspects of war and social aggression has been prepared by Mr. Thomas W. Huntington, secretary of the Committee for National Morale, and Dr. Ross Stagner, Dartmouth College. Copies may be obtained by writing to Mr. Huntington, 142 Chestnut Street, Boston, Massachusetts.

THE Child Study Department of Vassar College has received from the General Education Board a grant of $10,985 to enable it to expand its program of research in normal personality development. This gift is intended especially to further the preparation of motion pictures with a sound track demonstrating child behavior and the growth of normal children.

This research on personality, which was begun last year through the help of both the General Education Board and the Josiah Macy, Jr., Foundation, is carried on under the direction of Professor Mary S. Fisher, chairman of the department, and Dr. L. Joseph Stone, research editor.

Children will be photographed in the three schools where the undergraduates work: the Vassar College Nursery School, the Lincoln Center Nursery School, and the Poughkeepsie Day School. The pictures will focus on the variety and the uniqueness of personality patterns, all normal, yet each presenting different problems for parents and teachers.

The child study films will be made available for groups of teachers in various sections of the country through the American Council on Education, as has been done with other data gathered in the course of this research at Vassar. Dr. Stone has already taken 3000 still pictures and 15,000 feet of silent black-and-white film to demonstrate personality development in children.

CORRECTION

THE news note in the January, 1941, issue of the *Bulletin* reporting the death of Edouard Claparède was in error. The date is September 29, 1940.

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